

being and that further discussions and possibly further research be undertaken before the Commission is asked to make a final ruling.

Additional references

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Comment on the proposed precedence of *Ovula gisortiana* Passy, 1859 over *Cypraea coombii* J. de C. Sowerby in Dixon, 1850 (Mollusca, Gastropoda)
(Case 3220; see BZN **59**: 173–175; **60**: 218–220; **61**: 40–42)

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The concluding statement to my previous comment was excluded from the printed version (BZN **60**: 218–220). It is given here as it summarises my primary objections to Pacaud & Dolin's proposal: 'in the future by using appropriate techniques it may be possible to demonstrate that *G. gisortiana* is a subjective synonym of *G. coombii*, and that the type species of *Gisortia* would then be correctly known under that name. However, given the lack of systematic or other detailed work on *Gisortia* over the past 70 years, rather than passing references to this strange looking cowry, I believe that were this to happen systematic stability would be essentially unaffected'.

A major concern with Pacaud & Dolin's application is that they have failed to demonstrate whether any of it is necessary. So are the two species synonymous? In the last systematic reappraisal of this group Schilder (1930) considered that they were not. Currently there are no new published morphological data nor has there been any published re-evaluation of old data. Therefore, the suggestion that the species are 'probably conspecific' (BZN **59**: 173, para. 4) can be seen as nothing more than that. If Pacaud & Dolin are unsure then clearly their proposal is premature. I noted that Schilder's work was the most complete on this group. Pacaud (BZN **61**: 41, para. 3) has interpreted this to mean that I considered it to be 'the best work' and then goes

on to criticise it at some length. I have no opinion on the usefulness of Schilder's taxa. It is true that many are based on internal moulds lacking discrete characters and I suspect that Pacaud & Dolin may be right in regarding many of these nominal taxa as presently being undiagnosable or, more harshly, to be based on 'useless internal moulds'. Unfortunately much of the fossil record consists of specimens in this state of preservation. What do we do with it? Simply ignore it?

Despite the unwieldy systematics, Schilder's work is still the most complete treatment of the group. I certainly look forward to this group being thoroughly systematically re-evaluated. Without doubt it is required.

But what characters should be used? Pacaud (BZN 61: 41, para. 5) stated that the features developed in the callus may be variable in *gisortiana* and 'cannot be regarded as discriminant characters'. The obvious questions are: what characters, at what hierarchical levels and in which taxa? Pacaud makes no comment on this point, but curiously, in discussing a now non-existent specimen, he earlier (BZN 61: 40, para. 1) stated that 'this specimen possessed a large callosity on the dorsal face that differed from the type of *G. coombii*. This feature makes *G. coombii* closer to *G. gisortiana* than *G. tuberculosa*'. It appears that callosities (large bumps developed in the callus) do have systematic value in some cases.

Pacaud (BZN 61: 41, para. 5) responded to my suggestion that morphometric data might be the key to unravelling the systematics of this group, which, he admits, has strongly variable 'qualitative characters'. Unfortunately he provided a height/diameter plot purporting to demonstrate that: 1) *tuberculosa* and *gisortiana* are distinct and 2) that the holotype of *coombii* is closest to *gisortiana*. History has repeatedly shown that overly simplistic biometric plots of skeletal variables such as height/width have cast more shadow than light in molluscan systematics. Too often they have been adduced in support of shaky systematic treatments. Briefly, there are significant problems with this plot: 1) The 'species' height and diameter variables barely overlap; on this basis one might interpret *gisortiana* as larger specimens of *tuberculosa* displaying allometric growth, particularly as we know nothing about the relative ontogenetic ages of the specimens. 2) Crucially, both height and diameter measurements include the variably developed 'callosities'. This is perfectly illustrated by Pacaud & Dolin (BZN 59, p. 175, figs. 1, 2). 3) A recently discovered, undistorted, small specimen (l = 94 mm, d = 72 mm) of *coombii* from the type locality falls right within the main *tuberculosa* cluster, as one might expect from the regression line of the latter. Another specimen (BMNH 41604: l = ca 150mm d = ca 95 mm) would seem to plot indistinguishably from *tuberculosa*. 4) Other large specimens of *coombii* in The Natural History Museum, London, (e.g. BMNH 4604) show a wide range of proportions. In light of the above objections, I have chosen not to redraft Pacaud's plot with additional data points as I believe it to be systematically meaningless.

In short, there is a place for detailed treatment and discussion of morphology and systematics—a systematic paper in a systematic journal. Pacaud's reply fails to satisfy either demand.

With respect to nomenclature let us consider that future work will have adequately demonstrated that the two nominal species are synonymous. If so, then will use of the older name upset stability? I contend that it will not. Of the 25 references provided in support of this proposal by Pacaud & Dolin many are simply listings in illustrated or unillustrated taxonomic compendia. Indeed, the three most recently published

figures are in lavishly illustrated books dealing with Recent cowries and provided seemingly for their curiosity value. As Pacaud (BZN 61: 40–41, para. 2) has confirmed, specimens of this strange, morphologically and systematically poorly known cowry are rare both in France and England. This is important given the current very poor understanding of this genus and lack of recent work dealing with it. Therefore, priority should be maintained in this case. Indeed, it is likely that applications such as this, if upheld, will serve to diminish the Principle of Priority and might lead to petty arguments over the relative frequency of use of competing names in the literature. This is not what any of us, least of all the Commission, should usefully spend time evaluating.

It follows, therefore, that to give precedence to the junior name would be premature and for that reason I recommend that the Commission should not approve the proposals in BZN 59: 174.

Comment on the proposed conservation of *Melania curvicostata* Reeve, 1861 and *Goniobasis paupercula* Lea, 1862 (Mollusca, Gastropoda) by designation of a neotype for *M. curvicostata*
(Case 3232; see BZN 60: 109–112, 300–302)

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In addition to my earlier comment (BZN 60: 302), the contents of the original publications of *Melania curvicostata* Reeve, 1861 and of *Melania densicostata* Reeve, 1861 require a discussion.

1. In their application in 2003 (BZN 60: 109–112) Thompson & Mihalcik state: ‘the original figures and descriptions of both nominal taxa are virtually identical’. Contrary to this statement, the original text contains eight differences between the two species. The original descriptions (complete, but not necessarily in original order) are quoted here:

Melania curvicostata: shell ovately turreted, livid-olive, encircled towards the apex with a reddish line; whorls convex, longitudinally plicately ribbed, ribs curved, gradually fading towards the aperture; aperture ovate, slightly effused at the base interior tinged with purple.

Melania densicostata: shell subulately turreted, burnt-olive; whorls eight to nine, rather flat, the last obtusely angled; longitudinally densely plicately ribbed; ribs stout and comparatively straight ending abruptly on an obtuse angle of the last whorl; aperture rather small, ovate, interior very faintly tinged with purple.

The original figures show, very clearly, *Melania curvicostata* Reeve with curved ribs and convex whorls, and *M. densicostata* with more closely spaced straight ribs, flat whorls and a subangular periphery of the last whorl, just as the two syntypes figured by Thompson & Mihalcik. The applicants attribute these two syntypes to two different species, which supports Reeve’s original taxonomic judgement, contrary to long-held views in the subsequent literature.